December 26, 1955

Dr. P. R. Edwards
Box 185
Chambhee, Ga.

Dear Phil:

Thanks for your helpful notes on S. "java" N97. One of my students, Tetsuo Iino recently found an exceptional b,i: enx result from the familiar TM2 —x abony, and we are far enough along with it that I think we should combine its analysis with that of N97. This exception behaves so far as if it is H_1^b H_1^i H_2^{enx}, and differes from corresponding derivatives of N25 and N97 primarily in showing simulataneous (instead of alternate) expression of the i and the b. If the case holds up, it would be analogous to so-called "unequal crossing-over" which is known in Drosophila, and would help to understand the origin of N25. I hope to send you some of the derivatives to verify the serotypes, and will be grateful for your comment on them.

As to O-ϕ in abortus-equi, my reservation was only whether its occurrence should be ascribed to transduction, rather than apme other more interesting process— in fact we had some indications that ϕ might relate it to the lysogenic conversions of Uetake and Iseki. By the way, I just received a reprint from Iseki that the O-1 antigen is in fact converted in just this way— which is in line with my distrust of Kauffmann's offhand claim of "transduction" of O-1.

To turn to another subject,

I don't see what the linked transduction of two markers has to do with efficiency. If you are following a linked pair, the second marker will (by definition) go along in a high fraction of the transductions for the first one. Any given transduction is accomplished by only one phage particle. It does not seem unreasonable to me that the efficiency of transduction will vary widely from one system to another. My own experience would be quite consistent with a frequency of linked transductions of H_1-Fla_1 of about 10^{-6} (and with the right combinations, almost 10^{-5}) per phage particle.

What I wanted of you right away, Phil, was your possible advice about Dr. Frits Ørskov, and his wife Ida, from Kauffmann's laboratory. As I have hinted with you and Bill Ewing for some time, we would like to expand our efforts at immunogenetic analysis of E. coli by crossing methods. I mentioned this to Kauffmann also, and he is recommending that the Ørskov's apply for a fellowship to spend a year with me. My correspondence with the Ørskov's so far has been quite agreeable, and I would not hesitate to have them come on their own finances, but it may be necessary for me to find some local support in addition. Before I accept this responsibility, I would like to get any further advice or information that I can. Have you been acquainted with them? Can you suggest any one else who would be better suited to come here? Any ideas you may have on this would be welcome.

With best wishes for 1956,

Sincerely,

Joshua Lederberg